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IN THIS ISSUE

GREGG ON AEROLOGICAL RESEARCH

PAYNE ON GENETICS AND EVOLUTION

CIRCULATION THIS ISSUE OVER 16,000

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SIGMA XI QUARTERLY

VOL. 25 SEPTEMBER, 1937 No. 3

The 38th Convention of our society will be held in Indianapolis, December 28, 1937. A full agenda of the Convention will be given in the December issue of the Quarterly. Formal printed petitions for charters for chapters will be distributed to the chapters during the month of October for their consideration prior to the Convention. The Convention will be called upon to select a president of the society to serve for two years to succeed Doctor Durand whose term of office expires with this calendar year, a member of the executive committee for a term of five years to succeed Professor Leuschner, and a member of the alumni committee for a term of five years to succeed Mr. Donald H. Sweet. The nominating committee consists of Prof. Walter L. Upson, Washington, University, St. Louis, Mo., chairman; Prof. Ralph W. Chaney, University of California at Berkeley; Dr. Karl Compton, Massachusetts Institute of Technology. Chapters are requested to name their candidates direct to the committee, or through the office of the National Secretary.

RECENT ADVANCES IN AEROLOGICAL RESEARCH*

WILLIS RAY GREGG
Chief, United States Weather Bureau

Aerology is defined by Webster as "a, the branch of physics treating of the atmosphere." This is followed by "b, specifically, the description and discussion of the phenomena of the free air as revealed by kites, balloons, airplanes and clouds."

Talman[1] gives the following: "Aerology.—The branch of meteorology dealing with the 'free' atmosphere; *i.e.*, all parts of the atmosphere not near the earth's surface. Aerological investigations are made with kites and balloons, and also include observations of clouds, meteor trails, the aurora, etc." This is consistent with the definition proposed by W. Köppen at the Milan meeting of the International Commission for Scientific Aeronautics, in 1906, namely: "Aerology.—The study of the free atmosphere throughout its vertical extent, as distinguished from investigations confined to the layer of the atmosphere adjacent to the earth's surface; upper air research, as carried out with kites, balloons, etc., and through observations of clouds, meteors, the aurora, etc." This more restrained use of the term "aerology" and of its derivative, "aerological," has been generally adopted by meteorological services and is the one that will be followed in this paper.

Not many years ago it would have been perfectly possible to give a fairly complete summary of our knowledge of the structure of the free or upper air in the course of a single evening. This is no longer the case. Recently this subject and its application to the practical business of providing service for aeronautics and forecasts for activities of every kind, have prompted several educational institutions to organize comprehensive courses whose graduates therein are finding professional employment, both in the government services and with commercial organizations. These courses occupy never less than one year, but usually two, three or even more. They are made possible by the rapidly increasing amount of upper air data resulting from intensive investigations that are being conducted by practically all meteorological services throughout the world. Hence, the time happily has arrived when aerological research has become so extensive that only a small portion of the subject can be covered in the course of a single evening, and it is my purpose, therefore, to confine myself to what I believe to be the most significant advances of the last three to five years. These are:

(1) Winds in the stratosphere and sub-stratosphere.

(2) The use of upper air data in frontal analysis and therefore in forecasting.

(3) The development of the radiometeorograph.

* Address delivered before the Ohio State Chapter of the Society of the Sigma Xi, Columbus, Ohio, May 11, 1937.

Before taking up these topics, however, it is believed proper to summarize briefly what is known of conditions and phenomena beyond the present range of meteorological observation, that is to say, at altitudes greater than 30 to 35 kilometers above sea level. Our knowledge of conditions in the upper air strata is quite naturally in inverse ratio to their height above the earth. The characteristics of the troposphere or lowest portion of the atmosphere are known in considerable detail, those in the stratosphere up to 25 to 30 kilometers fairly well, though much is yet to be learned, but above this latter height little direct information is available. It is being rapidly added to, however. Only a brief summary of both fact and conjecture is here presented, and that summary is limited to phenomena that have, or at any rate appear to have, definite meteorological interest.

Attention is invited to Figure 1, which represents Martyn and Pulley's conception of the normal distribution of temperature up to a height of 300 kilometers.[2] The part of the curve up to about 30 kilometers, marked "Sounding balloon limit," is well established by observation and shows the troposphere with fairly regular decrease in temperature from the surface to about 11 kilometers, in this latitude, and above it the stratosphere, with an average temperature, also in this latitude, of approximately —55° C. There is a variation with latitude, the height of the tropopause (base of stratosphere) increasing from 6 or 7 kilometers in the polar regions to about 18 kilometers near the equator and the temperature of the stratosphere correspondingly decreasing from about —45° C. to —70° C.

Above a height of 35 kilometers no observations of temperature as such are available, and this part of the curve in Figure 1 is based on theoretical considerations and must be accepted as tentative. The marked rise in temperature between 40 and 60 kilometers, if it exists, provides an explanation of certain phenomena otherwise difficult to account for, chiefly, the action of meteors at these heights and the reception of sound waves beyond a zone of silence from explosions at points a long distance from their source.

The most interesting part of the curve perhaps is the rapid return to a low temperature at about 80 kilometers. This is based almost wholly on the occurrence at very rare intervals (or at any rate only a few observations have been made) of what are now known as noctilucent clouds. These consist of a thin, cirrus-like, luminous (presumably illuminated) veil in a layer of air having a movement of about 100 kilometers per hour. Humphreys[3] concludes that this cloud is composed of minute ice crystals and, if this be true, the temperature of the air necessarily must be low, approximately that shown in the curve. This conclusion has been accepted by Martyn and Pulley, as a result of an exhaustive research, already cited.[2]

These authors have extended their inquiry to still greater heights as indicated by the broken curves above 100 kilometers. While highly conjectural, they provide a reasonable explanation of the electron collision frequencies that are observed in the region between 200 and 300 kilometers. The high temperatures are attributed mainly to the absorption of solar ultra-violet energy by ozone and by ordinary oxygen.

Of special interest and possibly of considerable significance to meteorology are the variations which are found in the amount of ozone in the atmosphere.[4] They are in close correlation, for example, with the pressure, temperature and density in the stratosphere and with the barometric distribution at the earth's surface. This latter relationship is shown in Figure 2 from which it appears

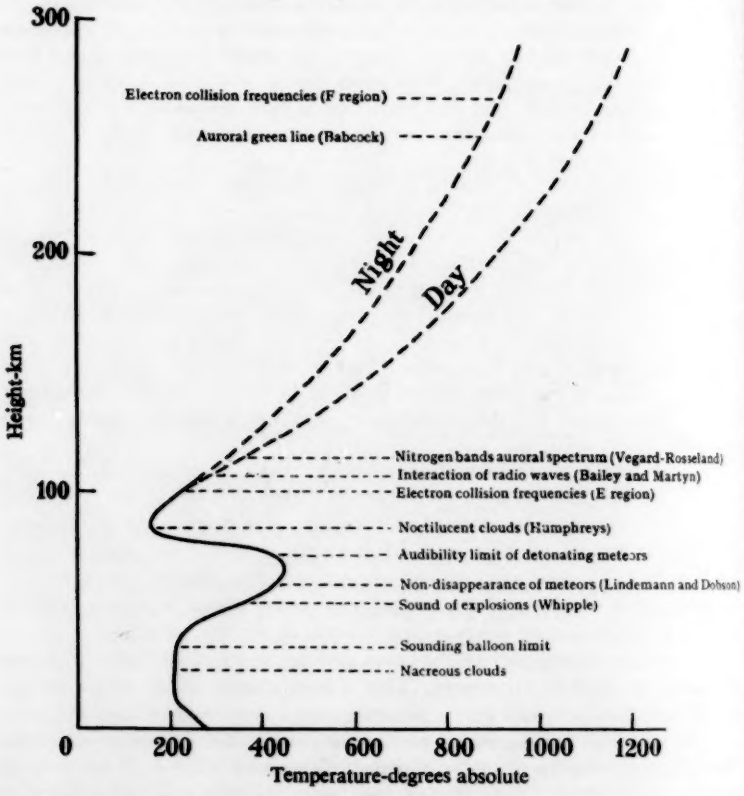


FIG. 1. Distribution of temperature with altitude. (After Martyn and Pulley.)

that ozone is greatest in amount a short distance to the rear of low pressure centers and least in the corresponding part of high pressure areas. It would seem then to be a valid assumption that the variations in the amount of ozone are related not to the barometric pressure itself but rather to the sources of different air masses, the ozone being most abundant in polar air and least in equatorial air.

The chief interest that all of this has to meteorology lies in the possibility of utilizing the measurements of ozone, and its variations, in forecasting the

development and movement of the different types of weather that are associated with fronts, anticyclones, cyclones, etc. It is significant that in 1935 the International Meteorological Organization, at a conference in Warsaw, adopted a resolution urging the organization of a widespread network of ozone stations for the purpose of determining whether or not knowledge of the variations in the amount of ozone from day to day may be of benefit in weather forecasting. It remains only to add that the Weather Bureau is planning to take part in this program by establishing five or more such stations in this country.

In this extremely sketchy outline we have barely touched upon a few of those phenomena in the superstratospheric portion of the atmosphere that are of interest and perhaps also of significance to meteorology. For those who are so inclined, the reading of what is known regarding other things in these regions of tenuous air will prove to be of absorbing interest. Our knowledge

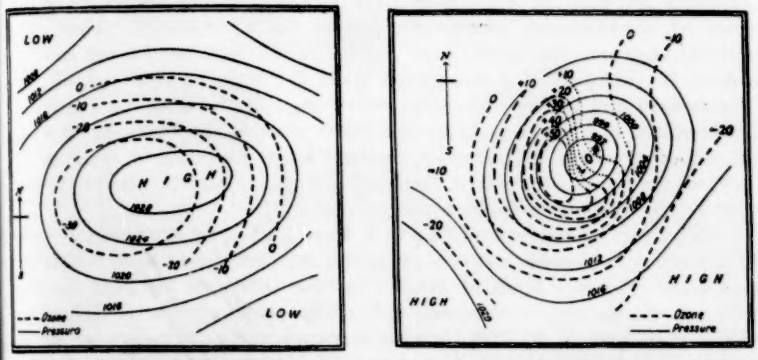


FIG. 2. Schematic distribution of ozone in cyclones and anticyclones. (After Dobson.)

of the far upper air is as yet very limited, but we are adding to it almost daily and at a progressively increasing rate. The state of this region is vital to radio, and it may have a greater influence on our surface weather than we now realize.

WINDS IN THE STRATOSPHERE AND SUB-STRATOSPHERE

This topic is a particularly timely one at present in view of the very definite trend toward flying at higher and higher levels. There is every reason to believe that, in the near future, nearly all long distance flights will be made at heights of 20,000 feet or more, thus getting away from the turbulent weather conditions of the lower levels and also taking advantage of the more favorable winds prevailing in the higher part of the troposphere and the lower part of the stratosphere.

Up to the present time the pilot balloon has been the chief source of information regarding upper winds. It has been used in this country for about twenty years. It has the weakness that it cannot be used when very low clouds are present or when precipitation is occurring. Moreover, when winds are

strong in the lower levels, the balloon is likely to be carried out of the observational range before the higher levels are reached. The data are therefore to some extent selective, although not to the extent that might be thought since soundings are frequently made just before or immediately following precipitation or the occurrence of cloudiness. Altogether therefore it is generally considered that the data represent quite closely the average or normal state of the atmosphere at all levels for which a reasonable number of observations has been secured.

Summaries and analyses of data for the lower half of the troposphere, up to 5 or 6 kilometers, have been presented from time to time, and the broad, general characteristics of this region, so far as winds are concerned, are quite well known. Sufficient data for that purpose become available in the course of so short a period as five years, although the longer the period the better. Naturally the data are less complete for higher levels but during the twenty years of observation a considerable amount has accumulated. These have recently been analyzed by Loyd A. Stevens of the Weather Bureau and will shortly be published in a monograph under the title "Winds in the Upper Troposphere and Lower Stratosphere over the United States."[5] The results are presented in a series of tables and charts showing the frequency of winds of various directions and velocities, resultant winds and extreme velocities for the four seasons of the year at levels of 6, 8, 10, 12 and 14 kilometers. We shall be able to give here only a sketchy outline.

Attention is invited first to Figures 3, 4 and 5, showing wind roses at 8 and 12 kilometers in summer and at 8 kilometers in winter respectively. Data are insufficient for higher levels in winter. In these charts the length of the lines indicates the percentage frequency of winds from various directions, the figures at the ends of the lines are their average velocities in meters per second, and the number of observations is shown in parentheses near the wind roses.

Conspicuous features brought out by Figures 3, 4 and 5 are: (a) the predominance of westerly winds in the northern half of the country during summer and in all sections during winter; (b) the wide scatter in the southern part of the country during summer, with easterly winds predominating in Florida and Texas, owing to the northward extension of the Trade wind regime; (c) the higher velocities of westerly than of easterly winds, except in the southern states during summer; and (d) in general the higher velocities of all winds in winter than in summer, resulting from the steeper horizontal pressure gradients at all levels during the former season, these in turn being due to the much larger contrasts in temperature than prevailing at different latitudes.

Turning now to Figures 6 and 7, we find the same data presented in a different form. They have been treated vectorially and the arrows shown, each for a different level, give the *resultant* wind value and therefore represent the movement of the atmosphere as a whole past a given point over a period of years (*e.g.*, given two vectors, north 10 and south 9, the resultant is north 1). Only the summer and winter values are given, but there are included all levels at 2-kilometer intervals, from 6 to 14 kilometers, for which sufficient data are available.

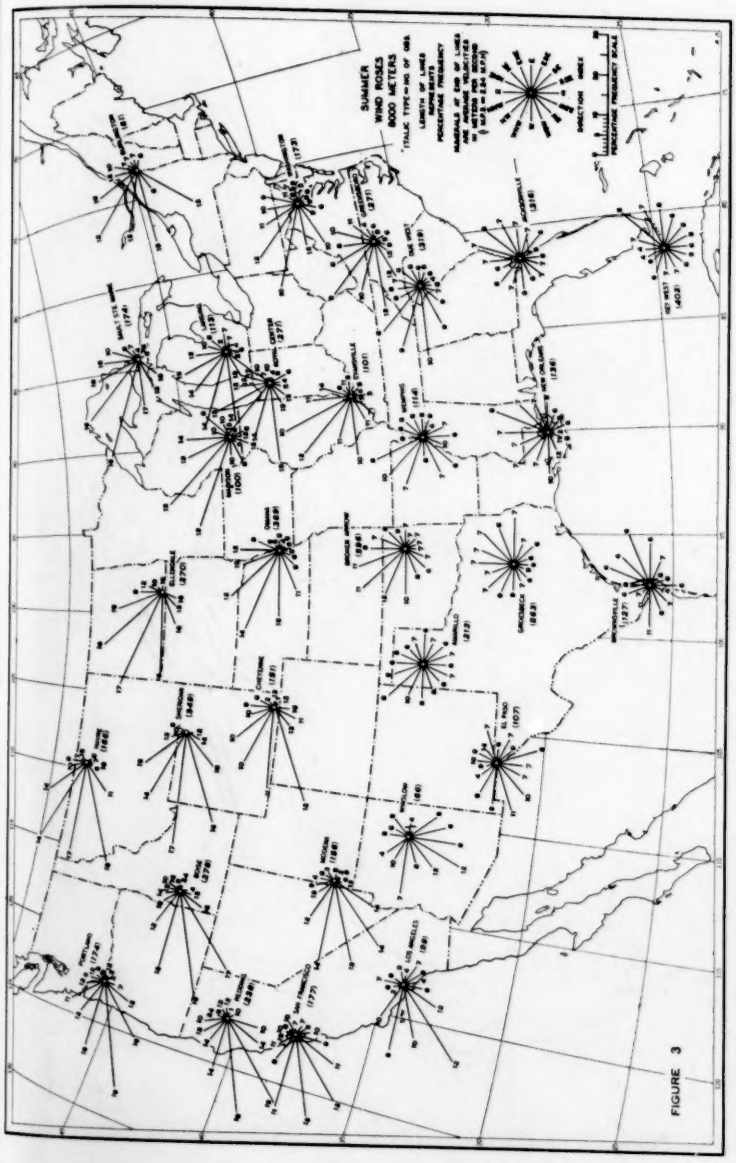
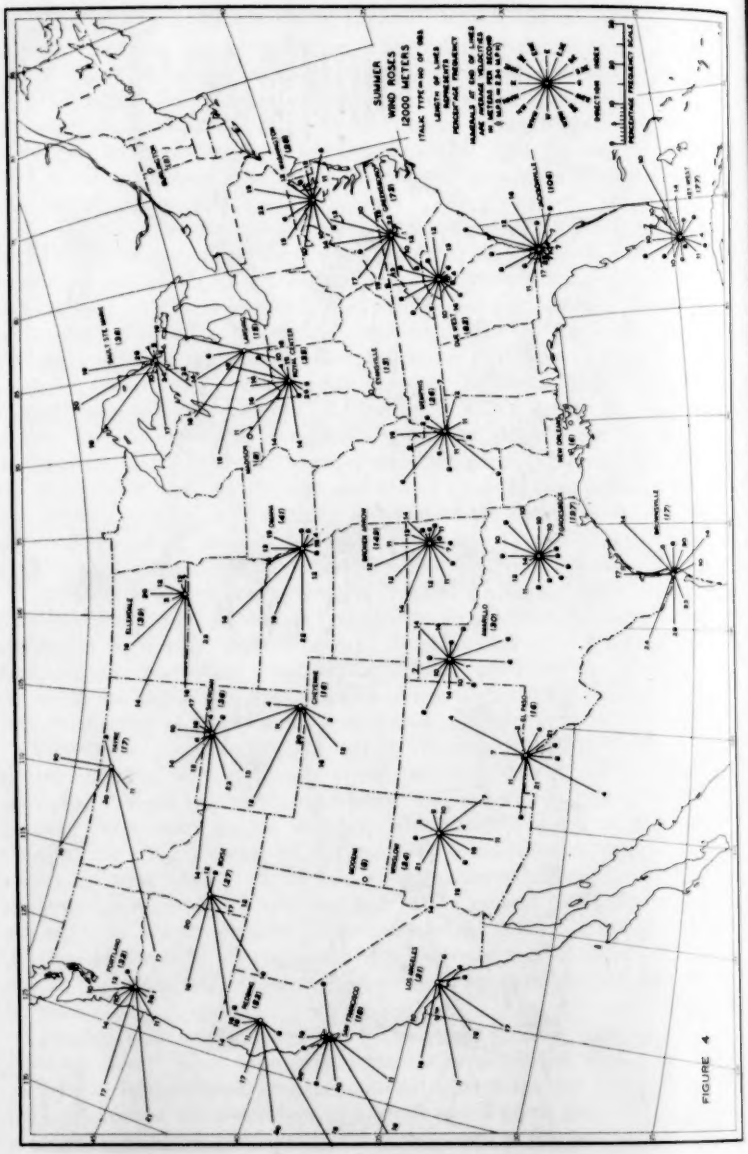

SUMMER
WIND ROSES
8000 METERS
(ITALIC TYPE = NO. OF OBS.
LENGTH OF LINES
FROM CENTER
PROPORTIONAL TO
PERCENTAGE FREQUENCY
NUMERALS AT END OF LINES
ARE AVERAGE VELOCITIES
IN METERS PER SECOND
(1 METER PER SEC. =
2.24 MILES PER HOUR)
DIRECTION ARROWS
PERCENTAGE FREQUENCY SCALE
SEATTLE (1724)
SPOKANE (1173)
HAVRE (1171)
MINOT (319)
GRAND FORKS (318)
DULUTH (403)
BOISE (1724)
HELENA (1113)
GREAT FALLS (1371)
BILLINGS (1101)
MILES CITY (1146)
BISMARCK (1245)
PORTLAND (1000)
OMAHA (1289)
NORTH PLATTE (1277)
SPEARFISH (1270)
DENVER (1212)
GOODRICH (1182)
SALT LAKE CITY (1270)
CHEYENNE (1181)
PUEBLO (1346)
EL PASO (1107)
WINSLOW (1178)
MODENA (1188)
RENO (1278)
YUMA (1740)
LOS ANGELES (89)
SAN FRANCISCO (1277)

FIGURE 3


SUMMER
WIND ROSES
(2000 METERS)
(ITALIC TYPE = NO. OF OBS.)
LENGTH OF LINES
INDICATES
PERCENTAGE FREQUENCY
NUMERALS AT END OF LINES
AN AVERAGE VELOCITY
IN KNOTS (1 KNOT =
1.15 STATUTE MILES
PER HOUR)
DIRECTION
PERCENTAGE FREQUENCY SCALE
FIGURE 4

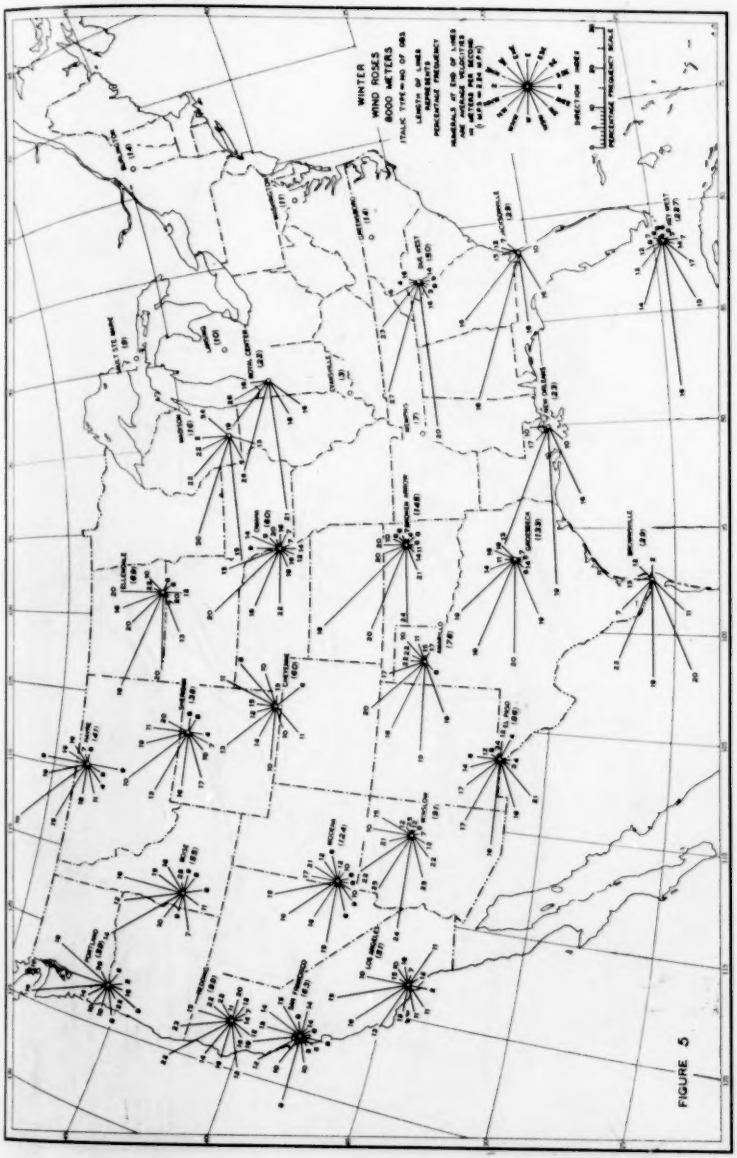

WINTER
WIND ROSES
6000 METERS
(ITALIC TYPE=NO. OF OBS.
LENGTH OF LINES
REPRESENTS
PERCENTAGE FREQUENCY
RADIALS AT END OF LINES
INDICATE DIRECTION
OF STRONGEST WINDS
(1 cm = 12½ m/s)
DIRECTION (DEG.)
PERCENTAGE FREQUENCY SCALE

FIGURE 5

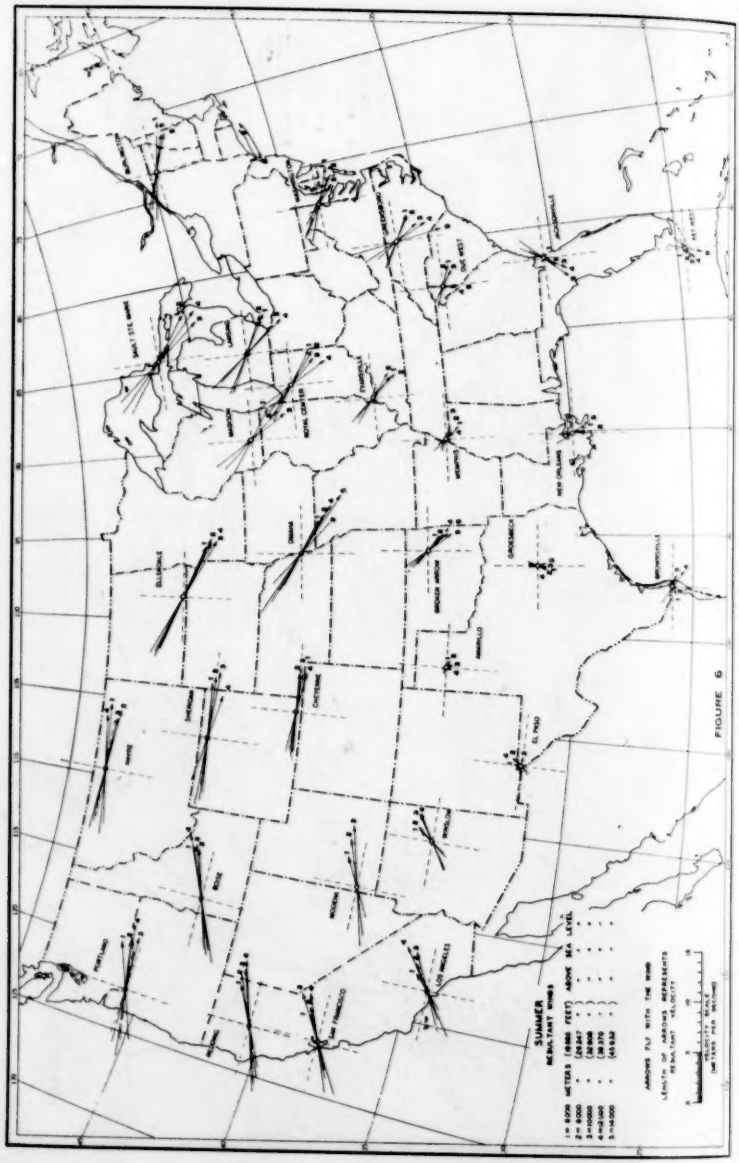

SUMMER
RESULTANT WINDS
ARROWS FLY WITH THE WIND
LENGTH OF ARROWS REPRESENTS
RESULTANT VELOCITY
VELOCITY SCALE
METERS PER SECOND
SEATTLE
BOISE
SALT LAKE CITY
RENO
HELENA
GREAT FALLS
MINNEAPOLIS
DULUTH
CHICAGO
DETROIT
BUFFALO
CLEVELAND
PITTSBURGH
NEW YORK
BOSTON
PORTLAND
CHEYENNE
DENVER
OMAHA
DES MOINES
MILWAUKEE
MADISON
MINNEAPOLIS
ST. PAUL
BISMARCK
MINOT
WILLISTON
ABILENE
WICHITA
KANSAS CITY
ST. LOUIS
OKLAHOMA CITY
AMARILLO
ALBUQUERQUE
EL PASO
SAN ANTONIO
HOUSTON
NEW ORLEANS
MEMPHIS
LITTLE ROCK
SHREVEPORT
JACKSONVILLE
ATLANTA
MONTGOMERY
MOBILE
CHARLESTON
RICHMOND
WASHINGTON
PHILADELPHIA
BALTIMORE
NEW YORK
BOSTON

FIGURE 6

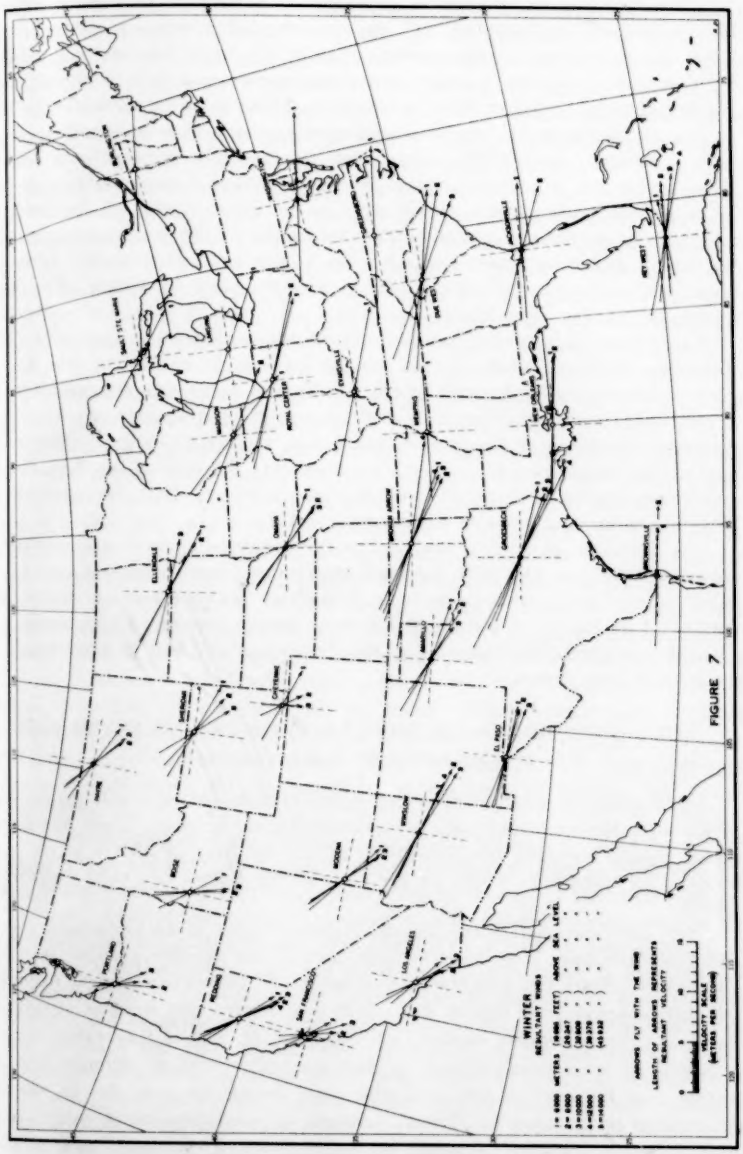

WINTER
MEAN UPPER WINDS
1 = 6000 METERS (19685 FEET) ABOVE SEA LEVEL
2 = 8000 (26247
3 = 10000 (32808
4 = 12000 (39370
5 = 14000 (45932
ARROWS FLY WITH THE WIND
LENGTH OF ARROWS REPRESENTS MEAN UPPER WIND VELOCITY
0
10
VELOCITY SCALE
(METERS PER SECOND)
SEATTLE
BOISE
MINNEAPOLIS
CHICAGO
DENVER
OMAHA
SALT LAKE CITY
RENO
SAN FRANCISCO
LOS ANGELES
EL PASO
ALBUQUERQUE
OKLAHOMA CITY
DALLAS
MEMPHIS
ATLANTA
JACKSONVILLE
NEW ORLEANS
MIAMI

FIGURE 7

Many features brought out in Figures 3 to 5 are here not only confirmed but emphasized, for example, (a) the existence of a west component at all levels and in all parts of the country during winter; (b) the lack of a definite air movement over the southern states in summer and (c) in general, the higher velocities in winter than in summer. There is an interesting exception to this last statement in the case of the most northern stations, winter showing lower resultant values than summer, and also a slight decrease with height. This may be due to failure to make observations when strong winds occur, the balloons being then carried out of sight in the lower levels, but another and altogether plausible explanation is that the height of the tropopause (base of the stratosphere) is considerably less in winter than in summer, probably about 8 kilometers in the former season over the northern states. This is a matter for further investigation.

Apart from their practical value in connection with the planning of air line schedules, resultant winds are of special interest in studies of the larger general movements of the atmosphere. In fact, if accurately determined from a sufficient amount of data, they indicate exactly the characteristics of the planetary circulation in the regions which they represent. A good illustration is given by the statement in (b) above, regarding the lack of anything like a definite air movement at upper levels over the southern states during summer. This is well brought out also in Figures 3 and 4.

The existence of a high level anticyclone, centered over Texas, is clearly shown in Figure 6, and fully confirms conclusions from theoretical considerations.[6] It and the eastward extension of the high pressure belt are due to the northward migration of the thermal equator during summer, which results in a similar northward displacement of the "horse latitude" belt to approximately the location here shown.

THE USE OF UPPER AIR DATA IN FRONTAL ANALYSIS AND THEREFORE IN FORECASTING

The issuance of weather forecasts is and probably always will remain one of the most important functions of a meteorological service. There are many others, equally valuable, but forecasting affects the lives of all the people and hence is the activity by which we are most widely known and most frequently judged.

Ever since national meteorological services began to be organized, about 1870, forecasts have been based in large part on the familiar synoptic weather map, which shows the geographic distribution of the meteorological elements and their changes from day to day. It is well known that weather conditions travel in a fairly regular manner, altering more or less as they move, and it is possible, from a detailed study of past synoptic charts, to estimate from a current map the changes that will take place during the next day or two in the location of cyclones or "Lows" (regions of relatively low pressure) and anticyclones or "Highs" (regions of relatively high pressure) and in the accompanying weather conditions that are characteristic of these pressure formations. Much progress was made during the early years of use of this

method, but, being empirical, a "dead line" of dependability was presently reached beyond which further improvement is not attainable.

During this early pioneer period many theoretical investigations clearly established the fact that it is not the cyclones and anticyclones as such that are important in determining weather, but rather the interactions between the various air masses that go to make up those systems of high and low barometric pressure. These air masses from different source regions, polar, equatorial, continental and marine, have distinct characteristics of temperature, humidity and movement, and they tend to maintain their individual identities over comparatively long periods of time, with the result that at their surfaces of contact they form more or less marked discontinuities or "fronts." It is along these frontal surfaces that the processes involved in weather phenomena are most active, and the study of them, now generally referred to as "air mass analysis," already has proved itself a powerful aid in developing forecasting along sound physical lines and is destined, we believe, in the not very distant future, to result in substantial improvement in the accuracy and precision of the forecasts and probably also in a moderate extension of the period covered by them.

The effective application of air mass analysis to the problems of forecasting was until recently not possible, owing to the lack of sufficiently detailed data at the surface and more particularly in the upper air. These deficiencies have in part, and in part only as yet, been overcome. Radio has greatly assisted by making reports available from sea and from distant continental sections. Codes have been adopted which yield greater detail in the data transmitted. Most significant of all perhaps is the introduction of the airplane in making upper air observations. Airplanes have their limitations as to height and regularity, but they are now providing at about twenty-five places in this country invaluable data up to about 5 kilometers, quite promptly and in all weather conditions except those that are too hazardous for flight. There is thus made available for analysis information regarding the vertical structure of the atmosphere, including its pressure, temperature and humidity, thereby supplementing observations of upper winds secured by means of pilot balloons and cloud observations.

The introduction of a new technique, especially when it involves the use of data of a type not previously available, requires careful study to determine the most effective means of presenting those data for rapid and thorough analysis. The "raw" observational material is useful in showing the current state of the individual meteorological elements, but offers little aid in an understanding of the energy transformations that are taking place. The latter is essential for the identification of the various air masses, for the analysis of their physical conditions and processes and for the prediction of their future behavior. For these purposes the more conservative properties of the air are of special importance; in seeking an index which will remain as nearly as possible constant, two quantities known respectively as specific humidity and equivalent potential temperature have been found most useful: The *specific humidity* is the mass of water vapor per unit mass of moist air; the *equivalent potential temperature* is the temperature to which air would come if subjected to a pseudoadiabatic lifting (that is, a lifting which is adiabatic except that all

condensation products drop out immediately upon formation) until all moisture was precipitated, followed by an adiabatic return to a standard pressure. Various procedures have been devised for bringing out, in diagrammatic form, the actual *thermodynamic* state of the atmosphere, among them the "tephigram,"

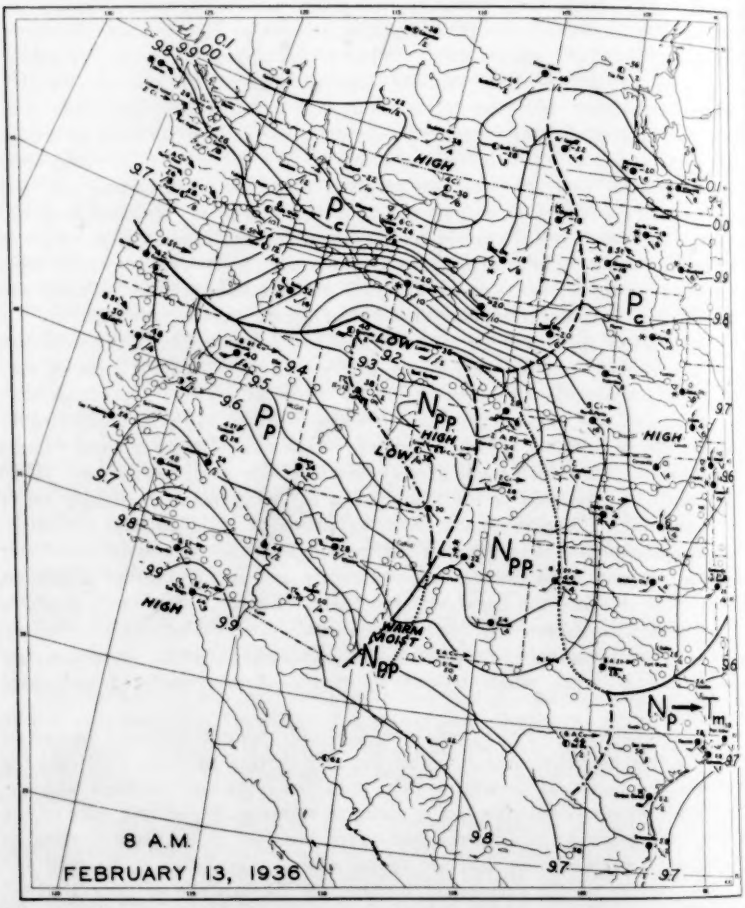


FIG. 8. Weather map of western United States, 8.00 A.M., February 13, 1936.

in which the absolute temperature is plotted as abscissa and the entropy as ordinate; the "Rossby diagram," with the mass of water vapor per unit mass of dry air as abscissa and the potential temperature of the dry air component of the atmosphere as ordinate; and the "emagram," which differs from the tephigram principally in the choice of coordinates, being plotted on an adiabatic chart with temperature for abscissa and pressure for ordinate.

Another highly useful form in which the upper air conditions can be quickly visualized is by means of the so-called "vertical section" which shows observed and equivalent potential temperatures, relative and specific humidities, winds, clouds, etc., as ordinates and a geographic section of the country, west to east, north to south, or any other direction, as abscissae. Necessarily the vertical scale is greatly exaggerated. Several such vertical sections are required to show the conditions for the entire country. We have not as yet been able to devise a method that would combine all into one chart, nor is it likely that we ever shall be.

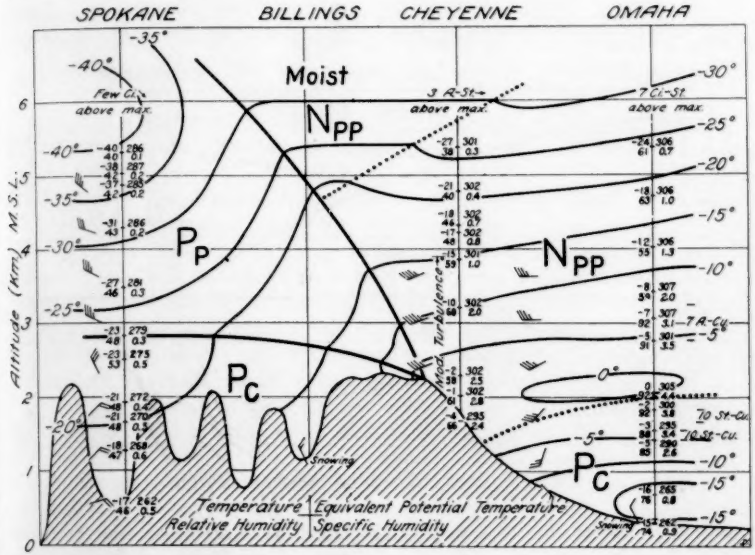


FIG. 9. Vertical section, Spokane-Omaha.

As an excellent example of the application that can be made of these vertical sections in air mass analysis, attention is invited to a recent paper by Lichtblau of the Weather Bureau.[7] In this paper the author discusses the formation, life history and other characteristics of upper air cold fronts in this country. A specific example is cited and several illustrative charts are shown, from which three are selected for use here as Figures 8, 9 and 10.

Figure 8 is the synoptic weather map of the western part of the country for February 13, 1936. It shows the usual features of such maps,—temperatures, winds, clouds, isobars, high and low pressure centers, etc., and in addition there are indicated the locations of fronts at the surface, solid lines representing cold fronts and broken lines warm fronts.

We come now to the part of Lichtblau's paper in which we are most interested this evening, namely, the use of upper air data in determining the location of fronts in the vertical and their relation to surface conditions. In Figure 9

are shown the temperatures* (observed, and equivalent potential), humidities (relative, and specific), winds, clouds and other conditions at various altitudes as determined from airplane observations at Spokane, Cheyenne and Omaha on February 13, 1936. Lines of equal temperature have been drawn and two cold fronts are indicated. In this figure as in Figure 8, to which it is related, the large letter symbols indicate the air masses that are present: P_P, polar Pacific; P_c, polar continental; and N_{PP}, transitional polar Pacific.

Figure 9 is a typical example of the way in which vertical sections are first prepared. For purposes of analysis it is desirable to place these as nearly as possible along a straight geographical line. Spokane, Cheyenne and Omaha being unsatisfactorily located in this respect, Lichtblau has used the data in

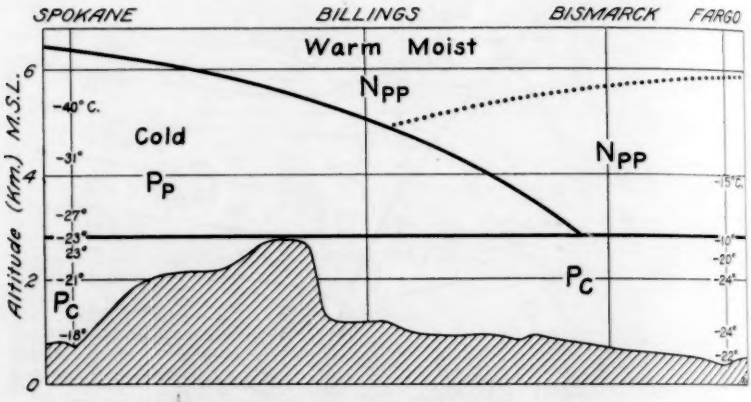


FIG. 10. Vertical section, Spokane-Fargo.

Figure 9, in conjunction with those in Figure 8, in constructing four schematic vertical sections, each in a different direction, of which one is here selected, Figure 10, which brings out in a very striking way the significant conditions along a west-east line from Spokane to Fargo. The author concludes his paper by pointing out the significance of these vertical sections in connection with the occurrence of precipitation.

Time does not permit our taking up the details of this paper. The point that we are trying to make is that a definite, substantial beginning has been made in the utilization of upper air data in the actual, serious business of forecasting the weather. The data are sketchy as yet, the technique of using them is in process of development and there remain many problems to be solved. Paradoxically enough, the more data we have and the more refined the methods of applying them the greater become our difficulties in meeting one of the requirements of forecasts, that is, the necessity of issuing them promptly. We should have difficulty, for example, in justifying a service that could provide perfect forecasts three or four days after the period to which they apply. There must always be a compromise between as near an approach to perfection as possible and the practical needs that we are endeavoring to meet. The important

thing is that we are developing this service along sound physical lines, supplementing at present as far as possible, and ultimately, we hope, largely replacing, the empirical methods that have heretofore been employed. Genuine progress already has been made, but it marks only the opening chapter.

THE DEVELOPMENT OF THE RADIOMETEOROGRAPH

In what we have said thus far it must have been evident that one of the greatest handicaps under which all meteorological services operate is the lack of adequate data from the upper air. At the surface automatic recording instruments provide a reasonably complete picture of the never-ending changes that take place. Continuous records in the upper air are not possible, perhaps never will be. It is necessary to depend on occasional soundings which represent only instantaneous conditions.

There is a still more serious deficiency, however. As already stated, pilot balloons are used for observations of winds, and airplanes for those of pressure, temperature and humidity. In addition, sounding balloons give us records of the latter three elements and, in some cases, winds also up to very great heights. Each of these methods of upper air exploration has serious limitations. Pilot balloons are useless in and above clouds and during precipitation, and they disappear quickly if strong winds prevail. Airplanes fail in bad weather, often when the data would be of most value, and they are limited to the lower half of the troposphere. Sounding balloons go very high but the records they secure do not become available until the instrument is found after its descent, and the information therefore cannot be used in current service.

These deficiencies in methods now employed and others formerly in use, such as kites, captive and manned balloons, provide the chief explanation of the feverish activity now evident in many quarters in the development of what is known as the radiometeorograph. At the present time seven independent investigations are in progress in this country, and similar work is being carried on in many other countries also. A fine feature connected with all this activity is the arrangement that has been made by the International Aerological Commission for exchanges of reports of progress, which include also detailed descriptions of the apparatus that each is developing. In other words, the goal is not individual glory, but the most efficient product possible. Thus, through this exchange of reports, the best features that are being developed by those now working on the problem will ultimately find a place in a few, probably not more than two or three, standard types.

It is not our purpose here to discuss technical details of these instruments. To do so properly would require very much more time and space than are available. Already the literature of the subject is fairly extensive, and those interested are referred to some of the more recent publications.[8] For our present purpose it will be sufficient to say that the radiometeorograph consists especially of a radio transmitter which is actuated by a barometer, a thermometer and a hygrometer, all of extremely light weight but of high precision. Nearly all models thus far developed in this country are bases on the Olland system of telemetering, in which a rotating hand, propelled by clock or other means, makes contact as it passes over small hands or levers whose movements

are controlled by changes in pressure, temperature and humidity. The intervals between these contacts and others that the moving arm makes regularly with fixed points are converted into values of pressure, temperature and humidity, on the basis of calibrations previously made.

The apparatus designed by Diamond, Hinman and Dunmore[8] departs from the Olland system and, instead of a clock or other rotating mechanism, utilizes the decrease in atmospheric pressure as the balloon rises for moving a small switch arm over a set of electrical contacts separated by insulating strips. Resistances in the circuit which are controlled by temperature and humidity elements modulate the continuous high frequency transmission of a radio oscillator, and by having the pressure element switch resistances in and out of the circuit in a definite order a record of all three elements is obtained at the receiving station.

The many perplexing problems connected with securing reliable records of pressure, temperature and humidity are in a fair way to complete solution, and we are now planning to put the instruments into regular daily use at two or more stations within the next few months. With the experience thus gained in the course of a year or two further improvements will be made which we are confident will enable us to substitute this method for airplanes throughout the service.

There still remain difficulties, however, in utilizing radio for giving records of air movement. In order to keep expense down to a reasonable figure it is desirable, if possible, to devise a method involving only one receiving station instead of two or more as ordinarily employed in radio direction finding. Experiments now in progress, using high frequencies, well up toward 200 megacycles, give considerable promise of success, so that, in all likelihood, within a few years, radiometeorographs will be capable of being used for obtaining records of wind direction and velocity as well as pressure, temperature and humidity.

Undoubtedly you have sensed, from what I have said, that in my view the development of the radiometeorograph stands at the forefront in the advances that have been made in aerological research, yet paradoxically enough it has as yet contributed practically nothing to the science. Why, then, all the enthusiasm and optimism regarding it? The answer is that the development has now proceeded to the point where we know that regular use will soon be possible. When that time comes we shall secure just the kind of information that is vital to detailed analyses of the structure of the atmosphere and the application of these analyses to forecasting. The data will not be restricted by meteorological factors limiting soundings as at present but will represent all types of weather conditions and reach well up into the stratosphere. The information will be available at once instead of two or three or more hours after the sounding is made.

Nor will the benefits be limited to our knowledge of the upper air. These instruments, or apparatus based on the same principle, can be placed on small uninhabited islands in the tropics to acquaint us with the development of hurricanes, or in isolated sections of the polar regions to give us advance information regarding the formation and intensity of a cold wave, or on moun-

tain summits to warn us of ice forming or other conditions hazardous to flying. The possibilities are in fact practically limitless and they abundantly justify our optimism in regarding this advance in aerological research as marking the beginning of a new epoch in the contribution of meteorological science and service to human efficiency, to human welfare and to human happiness.

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GENETICS AND EVOLUTION*

FERNANDUS PAYNE
Dean Graduate School, Indiana University

I have chosen to speak to you briefly on the subject of "Genetics and Evolution." Evolution is without doubt the central biological problem to which all others are subsidiary and so it is proper that we ask ourselves whether the recent advances in genetics, of which I spoke this morning, have contributed to a better understanding of the methods or theories of the ways of evolution.

Before proceeding, however, I wish to make clear the distinction between evolution and the method of evolution. By evolution I refer to the theory, or perhaps we might even call it a fact, that living organisms have not always been as they are and that they have come to be what they are, through a series of changes, from pre-existing forms. By methods of evolution I refer to the ways by which these changes have taken place, the causal factors involved, the directing forces. For the moment, then, we are concerned with methods of evolution. Evolution we shall take for granted.

In discussing these problems it is well to have in mind what any theory of evolution must explain. There exists several hundred thousand species of plants and animals on the earth. The concept of just what a species is, is not sharply defined, but for our purposes we may think of a species as a group of individuals having for the most part similar characteristics, living in the same geographic region and freely interbreeding. Our concept should also include variability among the individuals, genetic as well as somatic. Observations of living forms show that species are not static. They are ever changing, some perhaps more rapidly than others. The geologic record emphasizes this fact by demonstrating that species of the past were different from those of today. What is the cause of these changes? Are they random? What directs species in the way they go? If it is chance why do we not find many other species, for certainly all possibilities have not been exhausted. Why are there genera, families, orders, and phyla? Why has evolution, in instances like the horse's foot and the elephant's tusk, gone in definite directions? Some species have arisen, flourished for a time, and then disappeared. Why? Why do the principal phyla of animals as we know them today appear in that early geologic formation known as the Cambrian without any clearly established relationship? Why have organs, such as the eyes of the blind fishes, degenerated after having been functional in earlier phylogenetic history and why have the sensory touch buds become more highly developed? Why do not closely related species occupy the same areas? Why are some animals protectively colored, and why do some change their coats with the seasons? What is the explanation of mimicry? Of secondary sexual characters? Why are some animals and plants restricted to cold regions, some to warm, some to dry, and some to moist regions? Why do some animals feed on plants, others on other animals? Why are some forms parasitic, others free living? Perhaps they are adapted to the

* Sigma Xi lecture given in celebration of the seventieth birthday anniversary honoring the University of Kentucky's alumnus, Dr. Thomas Hunt Morgan.

conditions in which they live, but characteristics are not necessarily adaptive. In fact, some seem to have actually been injurious, if we may judge from such extinct forms as the dinosaurs. At least these highly specialized forms could not continue to exist under changed environmental conditions, even though these changes came about gradually. Even man continues to live on, in spite of wars, diseases, and the handicap of many physical defects and the presence of many vestigial organs. One might continue to ask many more questions for theories of evolution to answer, but perhaps these are sufficient to show you that the methods (note the plural) of evolution may be many, and that the operating causal factors may likewise be many.

Basic to all our thinking of evolutionary problems are two fundamental postulates which all take for granted. There must be change or variation, and secondly these changes or variations must be inherited. You see immediately, then, the intimate relations between genetics and evolution because genetics concerns itself primarily with variations and their inheritance.

What I propose to do is to look in summary fashion at the most popular theories of evolution in the light of our newer knowledge of genetics.

There have been two theories based almost wholly on the effects of the environment. The earlier one developed by Buffon, St. Hilaire and others thought of the environment as acting directly upon living organisms. As little was known of germ cells, the effect was upon the individual, molding it, so to speak, in one direction or another.

In the Lamarckian theory the effect of the environment is indirect. It causes the individual to do things which it formerly had not done. Use and disuse bring about changes which are passed on to the offspring, and thus acquired characters are said to be inherited. Both these theories are concerned primarily with the origin of variations.

The Darwinian theory of Natural Selection was based on several assumptions, as well as the observed facts that animals and plants vary and that man has brought about radical and diverse changes in domestic animals and plants by means of artificial selection and crossing. It was assumed that most of the variations occurring in nature are inherited. It was also assumed that, on the average, parents produce more than two offspring so that the population increases in a geometric ratio. It was further assumed that such a rapid increase in the population would give rise to considerable competition in the struggle for existence, to get food or to keep from being eaten. It was likewise assumed that some variations would occur which would be of advantage in the struggle for existence and that individuals possessing these favorable variations would more likely survive and reproduce offspring similar to themselves. The fittest would survive and hence would be adapted to the conditions under which they lived. In addition it was assumed that variations would continue, in the following generations, to occur in the same direction, that is, they would be cumulative. Subsidiary hypotheses were added to explain special cases such as secondary sexual characters and vestigial organs. Darwin even embraced Lamarckism in his later years to help explain the origin of variations.

The mutation theory as developed by DeVries was based upon observations and experiments with the evening primrose, Oenothera lamarckiana. I should

like to emphasize the fact that for the first time in the history of evolutionary thought the experimental method was effectively used. It is true that Mendel and other hybridizers had used the experimental method but they had not correlated their work with the existing theories of evolution. It is also true that Darwin used the method but only to a limited extent.

Oenothera lamarckiana is probably an American species introduced into Europe. DeVries found plants growing in the fields about Hilversum and discovered among these wild forms a number of variations, some of which were markedly different in several respects from the parental type. When brought into the garden and selfed, these variants bred true. The parent species, lamarckiana, when bred in the garden gave rise to more variations like those discovered in the wild. Since the variations appeared suddenly and bred true, DeVries called them mutations, and since the differences between mutants and parental forms were so great, he thought of the mutants as elementary species. He had discovered a method of species formation or evolution by means of large sudden heritable variations. DeVries called upon the Darwinian theory to supplement his theory of evolution by means of mutations, as the mutation theory alone concerned itself primarily with the origin of variations as did the Lamarckian theory.

Recently a new theory of evolution has emerged. I say emerged because it is called emergent evolution. It is not actually so new but it seems to have had a recent eruption, judging by the amount of discussion it has evoked. By some, including Jennings, it is hailed as a deliverer from bondage. What is this theory of emergence? I admit I am somewhat dense and so may get lost in the jungle of words with which it is enmeshed. If, however, I interpret correctly the adherents of the theory, an individual is not merely the sum of its parts. They illustrate it by saying that two separate gases, H and O, combine to produce water, which has properties entirely different from either gas. Water has emerged. Countless other examples of a similar sort could be given. New characteristics of animals and plants may emerge from genic changes and new combinations of genes, and so the door is opened for unlimited possibilities for evolution.

Paleontology has revealed many instances of evolution in a given direction. Notable examples already mentioned are the changes from a four-toed horse to a horse with a single toe and the progressive enlargement of the tusk of elephants. Many other examples could be given. In fact, if we look only at the end products, it would seem that all complex organs must have developed step by step in the direction they have gone. Eimer, who first developed this idea of evolution in a straight line, thought of the directive force or forces as within the organism. Others suggested that the environment might be the causal factor, while a third possibility is that only those variations which go in a given direction are preserved.

A factor in evolution, if not a complete explanation of species formation, is isolation. When we speak of isolation we usually think of geographic isolation, where barriers of one sort or another keep groups of individuals from freely intermingling. In the past it had been assumed that new variations had but little or no chance of survival because of the swamping effects of inbreeding

if the variation appeared in a large population. If, however, a variation occurred in a small group of isolated individuals, its chance of survival would be far greater. This idea was originally developed by Wagner, later by Jordan, and is now very generally supported by taxonomists. It can be illustrated best by a study of island faunas and floras. Volcanic islands must, of course, be populated by forms which migrate from the mainland. Darwin in his studies had noticed that the fauna and flora of the Galapagos Islands were similar to those of the mainland of South America, but that many differences existed. He also found that while the different islands of the group were very similar with respect to their faunas and floras, each island had forms peculiar to it. It would seem that isolation such as mentioned, while not encouraging variation, does possibly play a part in the preservation of variations and hence in this way, play a part in the evolution of species. Barriers may be of other sorts such as climatic, or physiologic differences, which prevent interbreeding.

The theories which I have outlined all too briefly do not exhaust the list, but they are the ones of major interest to us and time does not permit a consideration of others. You may have noted that no one of them gives complete and satisfactory answers to all our questions.

I have said that we are to look at these theories of evolution in the light of the recent discoveries in the field of genetics. I have also said that genetics is one aspect of the larger problem of evolution because without variation and inheritance evolution cannot take place. What are these developments in genetics which might influence our thinking about evolutionary problems? There are two aspects to these developments, one cellular, the other experimental, the cellular giving the physical basis for the interpretation of the experimental. With respect to the cellular basis we know:

(1) That an egg cell of maternal origin unites with a sperm cell of paternal origin and that from the fertilized egg a new individual arises by cell division, growth, and differentiation. Thus the germ cells are the only connecting links between parents and offspring.

(2) That each germ cell, maternal and paternal, brings in a series of chromosomes, so the fertilized egg has two series or double the number of the mature germ cell.

(3) That in cell division each chromosome divides so that every cell of the body has the double number of chromosomes.

(4) That in germ cell formation chromosomes pair, like with like, maternal with paternal, and then separate in the cell division which follows, each mature germ cell thus receiving the half number of chromosomes.

(5) That when the maternal and paternal elements of the pairs of chromosomes separate, the distribution is a random one.

From the experimental side a considerable body of knowledge has accumulated and certain laws or principles have been derived. The following are pertinent to our further discussions:

Characters are represented in the germ cells by means of elementary particles, genes. In the individual these genes are in pairs. Sometimes the

two genes are alike, in which case the individual is said to be homozygous. Sometimes they are unlike as in hybrids, in which case the individual is said to be heterozygous. In either case when the germ cells are formed the two genes of a given pair separate. This separation of the genes in germ cell formation is the law of segregation. The cellular explanation is that one gene of the pair lies in one member of a pair of homologous chromosomes and the other gene lies in the other chromosome of the same homologous pair.

In his original experiments Mendel discovered that when he crossed parents differing with respect to two pairs of characters, each pair behaved independently of the other. This is known as the law of independent assortment and is explained on the basis that one pair of genes lies in one pair of chromosomes and the second pair of genes lies in a second pair of chromosomes. The law is not of universal application as shown by more recent work.

One of the early discoveries of Professor Morgan and his students was that genes do not always assort independently; that sometimes the characters which go into a given cross together come out together. They called this linkage and the explanation is that the linked genes lie in the same chromosome. In the male Drosophila linkage is complete, but in the female linkage is only partial, the characters exhibiting recombinations in a certain percentage of cases. In the formation of these recombinations, crossing-over is said to take place and the assumption is that pieces of the homologous chromosomes actually change places. Genes which lie in the same chromosome show linkage in crossing over. Those which lie in different chromosomes show independent assortment.

One of the most interesting developments of the genetic studies in Drosophila is that the number of groups of linked genes is four, and that there are four pairs of homologous chromosomes. In corn there are ten pairs of chromosomes and ten groups of linked genes.

Studies of crossing-over have given evidence that genes are arranged in linear order within chromosomes, those lying near, crossing-over less, those lying farther apart, crossing-over more.

Other genetic facts pertinent to our evolutionary thinking are that in Drosophila and other forms where extensive studies have been made, many mutations have occurred and are still occurring. Most of these mutations are genic changes, but some are due to chromosomal rearrangements, or since the genes lie in the chromosomes, to genic rearrangements. It would seem that these mutations are random, that they are as apt to occur in one direction as in another, but certain mutations have occurred again and again, while others have occurred only once, and other possible mutations have never occurred.

Most of the mutations which have appeared are recessives, that is, when crossed to the wild-type, the wild-type character is dominant and so the hybrid looks like a wild-type.

While in general the effects of a gene are fairly definite, precise—due perhaps to the uniform conditions under which they exert their influence—still the environment may modify the effects of a gene. A good example is the red primrose which, when reared at 30° to 35°C. and in moist shady places, has white flowers. The same plants reared at 15° to 20° C. have red flowers. In

other cases the effects of the gene can be suppressed entirely. Abnormal abdomen in Drosophila, when bred in moist cultures, is a very marked defect, but when reared on dry food all flies are normal. Even though the character is suppressed for several generations by continuously breeding on dry cultures, the gene is in no way changed, for the moment moist cultures are provided, the abnormality reappears as before.

That the gene is stable is demonstrated by the fact that in hybrids genes separate sharply without influencing each other. In fact, a gene for white coat color may be kept in a black rabbit for generations without effect, for when two white genes again come together, a white coat is the result.

The popular concept is that a single character is dependent upon a single gene. Such a concept is far from the truth, for a character may be the resultant of a given combination of genes. The best known example perhaps is the cross between rose and pea comb in the domestic fowl. The hybrid is walnut, a comb unknown before this cross was made. In Drosophila orange eye color is due to two genes, one, salmon, the other, salmon modifier. But the probability is that every character is due to the combined action of a number of genes. If one pair of genes in the system changes, the modification or mutation which results, behaves, in inheritance, as due to a single pair of genes. Red eye color in Drosophila has mutated many times in different directions. Some of these mutant genes lie in the first chromosome, some in the second, and some in the third. Wherever they lie red always behaves as an allelomorph of the mutant gene. Now the gene for red cannot lie in different loci, unless there are several genes for red. The same is true for other characters.

While we usually think of a single gene as affecting only a given character, the fact is that a gene may affect several characters. The seed capsules of the common jimpson weed have spines. In Blakeslee's cultures a mutant with no spines occurred. These two forms not only differ with respect to spines, but also with respect to thickness of stems, length of internodes, and habits of growth.

The causes of genic changes have not been discovered, but mutations have been produced by means of heat and X and radium rays—some of the mutations which have occurred without treatment. These effects are direct effects on the germ cells or on the genes. While there is much speculation as to the cause of genic changes, the factual knowledge is so meager that such speculations have little value.

Another experiment, which may or may not be thought of as falling within the field of genetics, but which has had considerable influence on recent discussions of evolution theories, is that of Johanssen on Pure Lines. Darwin made use of artificial selection in building his theory of Natural Selection. It was well known in Darwin's time and is known now, that if, in bisexual forms, extreme variants with respect to a given character are selected and inbred, the mean for that character may be shifted in the direction of selection. There is a limit, however, as we now know, to such effects, and usually selection must be continued or the mean for the character reverts to what it was originally. In other words, change may be produced but it is not permanent.

Johanssen repeated this type of an experiment with beans, a form in which self-fertilization occurs. By using a single bean as the parent, in order to start with homozygous genes, and weight as the character, he discovered that he might shift the mean in the first generation, but after that, continued selection for lighter or heavier beans, was ineffective. The descendants of a single bean, he spoke of as a Pure Line. How, then, can evolution take place in Pure Lines? By means of mutation, Johanssen said.

Let us return now to our theories of evolution outlined earlier.

The Buffon, St. Hilaire thought was that the environment acting directly was the causal factor in evolution. Since, under experimental conditions, extreme heat and X-rays, by acting directly on the genes, may cause mutation, there is no reason why these same factors and others as well, might not account for some evolutionary changes if not for all. Even if we did grant, however, that environmental factors could account for all mutations—a supposition not supported by genetic evidence—there would still remain the question of the forces moulding evolution in the directions it has taken. Of course, the environment must play a large part, even here, but it is difficult to conceive just how the environment could determine the direction of the development of a complex organ like the vertebrate eye or ear.

The evidence from genetics gives little encouragement to the Lamarckians. Mutations appear suddenly. They are due either to genic changes or genic rearrangements and are in no sense due to somatic effects of the environment transferred to the gene. Genetics gives no evidence for any type of variation, other than mutation, as effective in evolution. It is true, of course, that the environment may cause mutations by acting directly on the genes, as just stated, but this is not variation in the Lamarckian sense. In spite, however, of the negative evidence of genetics, the theory still has its many followers. It seems so simple, so satisfying, where other theories fail. While many experiments have been made purporting to demonstrate the transmission of acquired characters, I think it may be truthfully said, even without a re-examination of the evidence, that not one of them will withstand repetition under the eyes of a critical investigator. It is true that somatic effects can be produced experimentally by changes in the environment; that such effects may even persist for a few generations after a return to normal conditions; and that the geneticists have not given a satisfactory explanation of this carry-over. Some biologists even think that if organisms are bred sufficiently long, millions of years perhaps, the effects might become permanent. Where, however, a known gene produces a given character, environmental changes which may affect the character, in no way affect the gene. In a selected mutant strain of eyeless flies there may be some flies with a few facets, but in most of the flies the eyes are completely gone. As the cultures for this strain get older, more flies appear with eyes and the number of facets is larger, yet when the flies with eyes are inbred and compared with cultures of flies with no eyes, the results are identical. The genes have not been affected by the changes in the soma.

In the original development of the theory of Natural Selection by Darwin, he made but little effort to explain the causes of variation. He saw variation everywhere in nature, and while he knew some variations were inherited and

some not, he more or less took it for granted that most variations were inherited. He knew that occasional sports or mutations occurred, but he thought they were too infrequent to be effective in evolution, and he also thought that such large sudden variations, or sports as they were then called, would tend to throw things out of balance, or cause a lack of coordination of parts. In his later years Darwin accepted the Lamarckian principle to explain the origin of variations. The variations Darwin saw and upon which he built his theory of Natural Selection were, for the most part, the small fluctuating variations which we now know to be due to the environment, and which we now know are not inherited. With respect, then, to the most crucial part of any theory of evolution—that is, variations and their origin—the theory of Natural Selection failed. It was built upon an insecure foundation. Considering, however, the weak foundation, the superstructure has withstood attack remarkably well, due largely to the fact that the error included the truth. Perhaps Darwin's error can be excused in part on the ground that the experimental method has not yet come into general use, and there is no way of distinguishing between fluctuating variations due to the environment and mutations except by the breeding test. It is true that genetics has not answered the question of the origin of mutations, but it has said emphatically that mutations are the only variations which can be used in the building of any theory of evolution, and that they are the result either of genic changes or genic rearrangements.

Even though Darwin's error was a gross one, it must be noted that the variations upon which he built his theory included mutations, as we define that word today—size does not enter into our definition—and so Natural Selection may still be, and probably is, a factor in evolution by operating upon mutations. In fact, Wright, in his recent discussions of evolution, is very positive on this question. He says, "The conclusion seems warranted that the enormous recent additions to knowledge of heredity have merely strengthened the general conception of the evolutionary process reached by Darwin in his exhaustive analysis of the data available seventy years ago."

Since Darwin thought of variations as random and varying about a statistical mean, the question arises whether mutations so vary. Are they as apt to occur in one direction as in another? Perhaps our data are too few for a final answer to such a question, but they are indicative. First, mutation frequencies are different for different genes. Some genes in Drosophila have mutated many times, others not at all. These mutations are usually in the same direction, but in some instances like the series of multiple allelomorphs of eye color, they are in different directions. But even here there would seem to be limitations, for the colors are variations of red, shading down by degrees to white. As Shull has pointed out, blue and green have not appeared. In this instance, at any rate, it would seem that variations in eye color can occur only within certain limits—limits determined by the structure of the gene itself and the physiology of the organism in which it operates. Even though mutations are not random, in the sense that they are as apt to go in one direction as another, they may still furnish the variations on which Natural Selection acts. It merely means that the possibilities for evolution in diverse directions will be limited.

Another aspect to the theory of Natural Selection is the assumption that variations are cumulative. That they have been cumulative in some instances, at least, no one can doubt; but the evidence from genetics—inadequate we admit—is negative.

Much discussion has hinged about the question of the chance of survival of a new variation in a large population. Prior to our present knowledge of genetics we talk about the swamping effects of crossing. The idea seemed to be that if a new variant mated to a normal or wild-type individual the new variation would be diluted; that if this hybrid mated to another wild-type individual there would be still further dilution; and that if this outcrossing continued long enough the new variation would finally disappear. On this basis a variation appearing only in one individual or in only a few individuals would have very little or no chance of survival. In the light of genetics this question is now an entirely different one. Mutations are due to genic changes or genic rearrangements. Genes and their effects are in no way diluted by continued outcrossing. They persist, even though recessive and in the heterozygous state, as long as the individuals possessing them persist, and they will be passed on to future generations as long as these individuals reproduce.

With respect to dominant mutations the case is simple. The only question is whether the mutants are able to survive and reproduce. With respect to recessive characters the answer is not so easy. Just what chance has a recessive character, assuming it to be viable, of establishing itself in a population, keeping in mind that two like genes must meet in fertilization for the character to appear. If the mutation appears only once, the chance of a recessive to establish itself in a population, and perhaps even replace the parental type, seems very remote indeed. If the same mutation occurs more than once, naturally the chances of survival would be increased. Since, however, such a problem cannot be attacked experimentally, the answer to such a question will probably never be reached. Only theoretical possibilities can be pointed out and this the mathematicians have done. It may be true, of course, that recessive mutations, particularly the kind which have occurred in our cultures—for the most part losses—have played little or no part in evolution; but this is a drastic statement, especially since genetics gives no conclusive evidence that other mutants are more potent in evolution. R. A. Fisher, in considering this question, devises ways by which recessive characters may be changed to dominant ones by the selection of modifying genes. Whether this is possible I shall not pass judgment, but Wright is skeptical, even though he thinks the recessive mutants furnish sufficient variations for evolution. I must admit I cannot follow the mathematical gyrations of these men, and so for this reason have no right to criticize their conclusions; but I have never been convinced that a theoretical answer so derived is necessarily in conformity with actual facts in nature.

Isolation—not merely geographic isolation—is considered by some, particularly the taxonomists, as an important factor in evolution, and I must admit their evidence is convincing. The thought is that in restricted groups of individuals, variations will have a better chance of survival than in larger

populations, due to closer inbreeding and to an absence of crossing back to the main population. Hence, evolution is encouraged.

Genetics has demonstrated that there is much heterozygosity existing in populations. Hence anything which would bring about inbreeding, or anything which would prevent free intercrossing in the population, would tend to segregate already existing genes. In other words, isolated groups of individuals may become different from the general population without the occurrence of new mutations. Of course, if new mutations did occur there would be a better chance of survival than in the population.

It should be kept in mind, however, that in a restricted population there will be fewer mutations than in the general population for the very obvious reason that there will be fewer opportunities. It should also be kept in mind that the mutations of our cultures are mainly losses and that close inbreeding soon produces homozygosity with its deleterious effects. Wright, in his studies on population size and evolution, concludes that small and large populations are not as conducive as those intermediate in size—small populations for the reasons mentioned, large populations because there is so little opportunity for the production of homozygous individuals.

DeVries' mutation theory, as previously stated, was based upon the appearance of large heritable variations or mutations. The changes were so large that the mutants were described as elementary species. Species formation was thus a simple process—a sudden change, the cause of which was unknown.

Genetics has shown very clearly that size does not enter into our definition of a mutation; that most mutations are really small and hence species do not suddenly, by a single jump, come into existence. The result is that the mutation theory is now merely a statement with respect to the kind of variation which can be effective in evolution. Other factors have had to come to the rescue.

Orthogenesis or straight line evolution is very obvious in numerous instances, even though we do not know its cause. Genetics, however, gives no evidence for orthogenesis. The only orthogenetic series, if the end products alone are studied, is the series of eye colors from red to white—changes of a single gene. But these changes did not occur in an orthogenetic series. White first rose from red, and all the others except eosin have arisen from red.

It is obvious that my remarks have been somewhat superficial and incomplete, but nothing else was attempted. I have tried to point out to you the contributions of genetics to evolutionary thought because Professor Morgan has played such a large part in these genetic discoveries.

REPORTS OF COMMITTEE OF AWARD OF SIGMA XI GRANTS-IN-AID

The committee of award of the Sigma Xi grants-in-aid held its annual meeting in the Faculty Club of Harvard University, August 6, 1937. Present were: Dr. W. R. Whitney, Professor Harlow Shapley and Professor Gary N. Calkins, of the committee, and Edward Ellery, the national secretary. Applications were received from twenty-two individuals, and action was taken as follows:

PROFESSOR ALAN ARTHUR BOYDEN, Rutgers University, $150.00. *Project:* Serological study of the relationship of Crustacea.

PROFESSOR LEOPOLD RAYMOND CERECEDO, Fordham University, $250.00. *Project:* Development of a 2-day test for the bio-assay of antineuritic concentrates in mice.

PROFESSOR FORREST F. CLEVELAND and PROFESSOR M. J. MURRAY, Lynchburg College, $300.00. *Project:* Raman effect and molecular structure.

PROFESSOR MAURICE EWING, Lehigh University, $200.00 (on condition that he get $161.25 from some other source). *Project:* Geophysical investigations of oceanic basins. (Since the meeting of the committee, Lehigh University has awarded Professor Ewing the amount of $161.25.)

PROFESSOR GEORGE WILLIAM HUNTER, III, Wesleyan University, $250.00. *Project:* Studies of the reaction of the host to the penetration of larval parasites.

PROFESSOR LOUALLEN F. MILLER, University of Minnesota, $250.00. *Project:* Solar radiation by spherical absorber.

MR. EDGAR J. MURPHY, College of the City of New York, $150.00. *Project:* Design, construction and calibration of suitable apparatus for measurement of X-rays and Gamma rays. Make a study of the stray radiation in the vicinity of the X-ray tubes and radium packs located in the various city hospitals. Design and construction of Geiger Muller tubes for measuring high intensity radiation.

PROFESSOR JENS RUD NIELSEN, University of Oklahoma, $200.00. *Project:* Raman spectra and structure of simple polyatomic molecules.

MR. AARON JOHN SHARP, University of Tennessee, $250.00. *Project:* Study of types and distribution of certain bryophytes and spermatophytes.

The committee voted that a list of the grants be sent to the Rumford Committee, the Permanent Fund Committee of the American Academy of Science, the National Research Council, and the American Association for the Advancement of Science.

W. R. WHITNEY,
HARLOW SHAPLEY,
GARY N. CALKINS.

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DIPLOMAS FOR MEMBERS AND ASSOCIATES

These diplomas are available in any quantity at 10 cents each. Orders should be sent to the National Secretary, should specify whether for members or associates, and should be accompanied by check.

INDEX CARDS

Index cards for newly elected members and associates are available *gratis* upon requisition from chapter secretaries to the National Secretary. These cards should be made out in duplicate, one set being retained for chapter files and one set being sent to the National Secretary for filing in the permanent records of the national organization.

NATIONAL CONSTITUTION

Printed copies of the National Constitution, containing all amendments to date, and all recent interpretations as made by the national officers on request of chapters, are available at 9 cents each from the National Secretary.

CHANGES OF ADDRESSES

Chapter secretaries are asked to send to the National Secretary in October of each year changes in their enrollment lists as follows: 1. Names and addresses to be deleted from the previous list; 2. Names and addresses to be added to previous list; 3. Changes of addresses of those on previous list who may have moved to a new address since the list was submitted.

SIGMA XI STATIONERY

Stationery in the official color of the Society is now available for all chapters and clubs at $1 per 100 sheets and $1 per 100 envelopes. The letter sheets bear the Society's seal embossed in white but no printing. The envelopes are the official square envelopes used by the national officers. Printed heading on the sheets and printed corner cards on the envelopes can be provided at cost, when so desired.

EDWARD ELLERY,
National Secretary, Sigma Xi.
Union College,
Schenectady, N. Y.

DEC 14 1937

SIGMA XI QUARTERLY

VOL. 25 DECEMBER, 1937 NO. 4

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GELLHORN ON PSYCHOPHYSIOLOGIC RELATIONSHIPS

STUDER ON THE URGE TO KNOW

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SIGMA XI QUARTERLY

VOL. 25 DECEMBER, 1937 NO. 4

THE 38TH CONVENTION

Date: December 28, 1937.

Place: Indianapolis, Claypool Hotel.

Hour: 4 P.M.

Delegates: Chapters are entitled to a representation of not more than three delegates. The delegates are chosen from the chapter membership, but if a chapter is unable to send its own members, it may appoint members of any other chapter as its representatives.

Clubs are entitled to a representative of not more than three delegates. Club delegates have the privilege of the floor, to discuss any subject before the convention. They vote only on questions directly affecting alumni members and associates.

Chapters and clubs are asked to appoint delegates and report their names to the National Secretary not later than December 14.

Business:

Reports of National Officers.

Proposed amendment to the Constitution:

Article II. 4. A petition that a charter of an established chapter be revoked shall be referred to the presi-

dent of the society, who shall refer it to the Executive Committee for consideration. If the Executive Committee present the petition to revoke a charter to the convention with recommendation to favorable action, a charter may be revoked by a three-fourths vote of the convention.

Petitions for charters and chapters:

a. Rice Institute.
b. Wellesley College.
c. Massachusetts State College.
d. University of Florida.

These petitions were sent to chapter secretaries in October.

Election of Officers:

A president to succeed Dr. Durand, whose term of office expires December 31.

A member of the Executive Committee to succeed Professor Leuschner, whose term of office expires December 31.

A member of the Alumni Committee to succeed Mr. Sweet, whose term of office expires December 31.

The nominating committee consists of:

Prof. Walter L. Upson, Washington University, St. Louis, chairman.

Prof. Ralph W. Chaney, University of California, at Berkeley.

Dr. Karl Compton, Massachusetts Institute of Technology, Cambridge, Mass.

Chapters are requested to name their candidates direct to the committee or through the office of the National Secretary.

THE 16TH ANNUAL SIGMA XI LECTURE

Date: **December 28.**

Place: **Auditorium, Murat Theatre, Indianapolis.**

Hour: **8.30 P.M.**

Lecturer: **Dr. Irving Langmuir, Research Laboratory, General Electric Company, Schenectady, N. Y.**

Topic: "The Biological Applications of Surface Chemistry."

THE EXECUTIVE COMMITTEE

A stated meeting of the Executive Committee will be held in the Palm Room of the Claypool Hotel, Indianapolis, December 28, at 2 p.m. The National Secretary will present to the Committee all items of business which chapters submit for its consideration before December 21.

THE PRESENT STATUS OF CLINICAL INVESTIGATION*

WILLIAM S. MCCANN
Professor of Medicine
University of Rochester

The worker in pure sciences, accustomed as he is to extraordinary precision in the measurement of his data of observation and the utmost rigidity in the control of experimental conditions, must frequently be amazed at the temerity of the clinician, who ventures to apply at the bedside of a patient the methods of science. His data of observation are admittedly crude and often purely qualitative. His attempts at control of experimental conditions in organisms as complex as human beings must often seem little short of ludicrous to a mind like that of Willard Gibbs. Yet the practice of medicine has been such a potent stimulus to the curiosity of men, has sent so many physicians exploring into natural phenomena that Medicine has been aptly called the Mother of Sciences. We may say this proudly but without boasting or making undue claim for a place for Medicine among her progeny.

We may, it is true, speak of the medical sciences, but for the purposes of this discussion I would have you make a distinction in your minds between these disciplines and Mother Medicine herself. Anatomy is not Medicine, nor is Physiology or Biochemistry. The science of Pathology has grown to a mighty structure, dealing with diseases, their nature, causes and results, yet pathology is not Medicine, because Medicine deals not only with diseases but with the sick man, and in performing its ultimate functions it still remains an art.

It is my purpose tonight to talk about clinical investigators, those men who have brought the tools of science to the bedside with the aim of studying the interaction of the patient and his disease, in the hope of establishing a clinical science, which will strip from the art the last vestiges of mysticism, empiricism and guesswork. I want to make you feel that there is after all a kinship between you and these men. Their goal seems far less likely to be achieved than the final conquest of Mt. Everest. One is tempted to pursue the analogy, but in order that it may not be labored it will suffice to say that no matter what scientific preparations are made at the base camps, or what scientific aids and instruments are used, the final ascent of Everest will probably be made by mountaineers, that is by experts in the art of climbing mountains. And so it will probably be with the ultimate triumphs of clinical medicine.

The interactions, in that psycho-biological entity which we call a patient, between a pathological process and the patient himself, is revealed by what we call symptoms and physical signs. The beginnings of a modern clinical science were the foundation of nosography which was laid by Sydenham. The Greeks, of course, had made great progress in nosography. Sydenham revived the

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method of describing clinical entities by carefully observing signs and symptoms of the patient, their permutations and combinations and the sequences by means of which the course of disease was revealed. Sydenham's great merit lay in the fact that he substituted direct and careful observation for speculation, and thus painted accurate pictures of disease.

The idea of systematic classification of nosographic entities stimulated others. Linné developed such a system on the lines of botanical classification, but his efforts yielded no important result because he classified symptoms rather than diseases. Advance beyond the method of Sydenham had to await a period of pupation in nosography, which time pathological-anatomy was developing and clinicians were employing new techniques of observation.

Auenbrugger began to use immediate and later mediate percussion, and Laennec to use auscultation. Thus physical signs were added to subjective symptoms and superficial inspection as a part of the armamentarium of the nosographer, and, as clinical investigators sought to discover the meaning of these signs by correlating them with post-mortem findings, the morbid anatomy of disease became an essential part of nosography. This process initiated by Laennec provided the Leit-Motif of clinical investigation for more than seventy-five years, resulting in recognition of many clinical entities which now bear the names of the men who first described them. Thus we have Grave's disease, Addison's disease, Bright's disease, the Stokes-Adams syndrome, etc. Sometimes the first classical description of a symptom or sign was memorialized with the name of some keen observer, such as "Corrigan's water-hammer pulse," or "Cheyne-Stokes' respiration."

Almost the whole of medical progress in this direction up to 1870 was confined to France and the British Isles. During this same period in Germany the nosographic method was largely ignored. Pathological physiology which had received its first great impetus from Magendie and later Claude Bernard, caught the German fancy of the period. While much of the ground work of physiology, biochemistry, and pathology was being laid down in Germany during this period true clinical investigation languished. German clinical medicine did not begin to come to the fore in this field until after 1870. The return to the discipline of nosography initiated the great period of German medicine, producing such men as Frerichs, Leyden, Naunyn, Erb, Westphal, Friedreich and Kusmaul, men who may take their place among the great clinicians of all time.

About this time the impact of Pasteur's discoveries began to be felt. Bacteriology was brought to the bedside with the result, that in the infectious diseases, the specific etiological agent was added to the existing nosographic criteria of diagnosis. Such differentiations as that between typhoid and the paratyphoid fevers became possible, and to this might also be added serological criteria such as the Widal agglutination test in typhoid, or the Wassermann complement fixation in syphilis. After all the essence of the method of Sydenham and Laennec remained unchanged, nosography grew chiefly through refinements in the methods of observation.

Throughout the bacteriological period there was a great development especially in Germany of physiological methods in clinical observation. Not only were functional tests added to the general nosographic picture, but the impetus was strong to seek an understanding of the functional significance of symptoms and signs. While such a trend is discernible in German medicine from the beginning of the nineteenth century it was ineffective in producing a clinical science until given proper orientation through nosography, such as that which it received in the period from the Franco-Prussian War to the World War.

During this period the flourishing state of all the physical sciences and of the basic medical sciences resulted in a great enrichment of the armamentarium of the clinical investigator. The availability of the x-ray, the string galvanometer, the refinements of morphological, bacteriological and immunological techniques, and the improvement in biochemical methods, made possible a tremendous advancement in knowledge of pathological physiology. One may readily see the extent of progress in this direction by examining Ludolph Krehl's book on "Pathologische Physiologie" which is a fair summary up to the time of the World War.

Inseparably correlated with this growth of pathological physiology is experimental pathology. The attempts to produce disease states experimentally, apart from the infectious diseases, is well illustrated by the experimental pancreatic diabetes of the dog produced by von Mering and Minkowski. The procedure of partial pancreatectomy provided a host of biochemical investigators with a means of studying the disturbances in the intermediary metabolism of fat and glucose and greatly enriched our knowledge of diabetes. It also furnished the basic fact upon which the ultimate isolation of insulin depended.

It is almost always the case that the experimental disease falls short of exact duplication of the disease in man. Experimental pancreatic diabetes was no exception. It is only by careful comparison with the true nosographic picture that the limitations of the experimental disease may be appreciated. It is thus of the utmost importance that skill in clinical observation be not neglected by those engaged in experimental medicine and that the discipline of nosography be retained lest the experiments shoot wide of the mark.

The point may be further illustrated in the field of Bright's disease. The original nosographic descriptions of Bright made 100 years ago were quite accurate and complete. Bright described the gross anatomical lesions of the kidneys together with the principal types of clinical behavior of those afflicted. His description included the abnormalities of the urine, the accumulation of urea in the blood, the paucity of blood proteins, the phenomena of dropsy, hypertrophy of the heart, the hardness of the pulse (hypertension). The chief details which were lacking were those which required the aid of a microscope. These details were later added by his successors but in such profusion that the clear-cut outlines of the nosographic patterns were lost. It was not until 1914 that Volhard and Fahr in Germany rescued the subject from confusion and restored the clear sharp outlines of the clinical entities which make up Bright's disease.

Up to the time of this reorientation a vast amount of relatively futile work had been done in the experimental pathology of renal disease. Almost immedi-

ately after it progress began to be made in the experimental field. Recently Masugi in Japan, Ahlström in Denmark, and Smadel in the United States have been approaching ever more closely to the production of a progressive renal disease, resembling the diffuse glomerulonephritis of man, by the use of specific cytotoxic sera. Clinical investigators in this field will be satisfied with nothing less than complete identity of the nosographic criteria which mark the goal toward which they are struggling. Only in this manner can experimental data be obtained which may with confidence be applied to the disease as it occurs in man.

I hope at this point to be clearly understood. I do not decry investigations which are not inspired at the bedside. On the contrary I regard it as of fundamental importance that investigations should proceed from purely morphological, biochemical or physiological motivation without reference to the needs or demands of the clinic. The task of the clinical investigator is to use the tools thus provided to forge the last link in the chain, but that link if it is to fit must be moulded to the pattern of the true nosographic entity.

If I were asked what characterizes the newest development in clinical investigation in this country I would hazard the statement that it is the study of the patient as a psycho-biological entity. The sufferer from Bright's disease is not a decerebrate animal to be considered only as the possessor of a pair of kidneys. Even from the strictest scientific standpoint he is to be regarded as a thinking, feeling person, capable of emotional reactions which may profoundly influence all his vegetative functions, including those of his kidneys, just as the failure of his renal function may affect his psychic behavior and the whole integrative action of his nervous system. These psycho-somatic interrelationships constitute the peak of Mt. Everest which the clinical investigator must ultimately ascend.

The trouble with experimental pathology is that it still exists at the veterinary level. While dogs and cats have psyches, and exhibit bodily changes in hunger, fear, and rage, for experimental purposes they may be carefully isolated from influences which excite these emotional mechanisms. When we work with a patient in the clinic we rarely know to what extent his fear of death, his worry over taxes and the payment of his rent, his jealousies, loves, and hates or his thwarted ambitions, may be modifying bodily functions. It is characteristic of the modern physician that he takes these factors into account in his therapeutic program, yet they still remain a source of major embarrassment to the clinical investigator who is trying to maintain constant conditions during experimental observations.

I am reminded of the case of a physician who entered our metabolism unit as the subject of a study of diabetes. This man had been carefully observed during his active practice and experience had shown that even with restriction of his diet to 2,000 calories he required from thirty to forty units of insulin daily to control his glycosuria. This patient was "high strung" and in many ways erratic and irascible in his behavior. When he was strictly confined in the metabolism ward, isolated from the usual sources of emotional excitation, we discovered that he could tolerate a diet of 4,000 calories without any insulin

whatsoever. This is the sort of thing I had in mind when I said that the experimental diabetes of the depancreatized dog falls far short of duplicating the disease as it occurs in man.

I do not mean to imply that I despair of ever being able to control these factors. The clinical investigator may have to wait till the modern psychologists have mapped out hitherto unknown nervous pathways by means of action currents; until emotional reactions can be measured quantitatively, or until pharmacologists have taught us by the use of selective drugs to eliminate reactions which will interfere with experimental conditions.

Modern investigators are realizing the importance of Hippocrates' insistence upon a knowledge of the "constitution" of a patient. Efforts are under way to discover anthropometric clues to constitution and the basic psychological and physiological patterns which accompany them. So far little progress has been made in this direction. Perhaps the clues lie in the field of genetics rather than in anthropometry.

In the evaluation of etiological factors in disease, or the results of special therapeutic procedures the investigator must apply the calculus of variations to data obtained from sufficiently large and normally balanced populations in order to determine their effects. When dealing with such variable subjects there is no other recourse to be had but to calculate standard deviations and probable errors. Such procedures are dear to the hearts of actuaries but are accepted under protest by the true clinician who is trained to deal with his patients individually. Except as special procedures may be evaluated in this way there is little science in therapeutics, concerning which we must still say with Hippocrates that "Life is short—The Art is long—Opportunity is fleeting—Experiment is perilous—Decision difficult."

I have in this rather summary fashion tried to present to you some conception of clinical investigation as distinguished from that which is more purely of the laboratory. Some may think perhaps that I have overstressed the importance of the purely descriptive discipline of nosography. If I have done so it is because it has been such a useful guide, which in the past has kept experimental procedures from going futilely astray. It is like a map which shows the unexplored areas where details are to be filled in. Science proceeds both by observation *and* experiment, yet we are sometimes prone to slight the disciplines which develop observation, without which experiment frequently remains sterile.